LETTERS TO THE EDITOR.

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New Use for Eucalyptus.

THE following extract from a private letter deserves a wider publicity :—

W. T. THISELTON-DYER.

You may perhaps remember the work published in 1902 from this institution on "Eucalypts and their Essential Oils." Prior to this the eucalyptus oil industry was in a chaotic state in Australia, the New South Wales article being almost unmarketable. By working out the species on a basis as laid down in the work (*supra*)—that is, a natural one—many new products were discovered, such as geraniol, the active principle in the otto of roses; a dextro- and a lævo-turpentine corresponding to the American and French respectively; many eucalyptol oils (medicinal), in addition to those previously known; citral, from which ionone, the artificial perfume of violets, is made; citronellal, corresponding with the product obtained from the lemon grass of India; eudesmin and other products of unknown economic value at present.

The British pharmacopœia laid it down that no eucalyptus oil should be sold unless it contained not less than 50 per cent. eucalyptol, but such did not hold good for Australia; consequently *any* oils were sold for therapeutic purposes.

But recently, through our instrumentality, the Health Board has passed a regulation that the B.P. standard should obtain here; consequently there has been a "slump" in the oils thus disqualified, and so these became worthless. Naturally the distillers were very much put out with us, and some travelled so far as Victoria and Queensland to interview us and discuss the matter. But the hands of the clock could not be put back.

The now discarded oils have been classified by the chemist as phellandrene oils, and the particular gum trees yield them in larger quantity than any other oils, and the desideratum was to find an industrial avenue for their utilisation. Well, this is how it has come about. There are at certain large mining centres here millions of tons of "tailings," containing particles of minerals very finely divided, and the trouble in the past has been to extract these profitably, and many methods and patents have been adopted, but none satisfactorily.

At Broken Hill, our greatest mining centre, chemical investigation has been carried on for some time, and a complete series of essential oils of our own extraction and true to botanical names was forwarded for trial. The result was that the phellandrene oils yielded from 86 to 90 per cent. concentrates, the highest of any oil experimented with! Here indeed was a market. The demand for these particular oils has at once enormously increased; hundreds of tons are required, for the method will be introduced into U.S.A., Canada, Queensland, and through all Australia, South Africa, and wherever the industry obtains.

Its utilisation is the essence of cheapness, and the information on a sample that I brought from Broken Hill myself reads as follows :—Zinc concentrates; about Zn 47 per cent., Pb 10 per cent., Ag 15 oz.; recovered by eucalyptus oil ½ lb. per ton of concentrates.

When we started out on our research on eucalypts we little thought that the oil would play so important a part in mining. RICH. T. BAKER.

Technological Museum, Sydney, April 7.

The Date of the Discovery of the Capillaries.

THE discovery of the capillary blood-vessels being an event of such supreme importance in biology, it is highly desirable that the date of it should be accurately ascertained. It so happens that it is known for certain it was made in the year 1660 by Marcello Malpighi in the city of Bologna, and yet nearly every author gives the date of this discovery as 1661.

The latest statement of 1661 is in Prof. Miall's delightful little book "The History of Biology," in which in the useful chronological table we find :—

"1661. Passage of blood through capillaries observed by Malpighi." As justifying this date, he would doubtless point to the note at the head of the table :—("The date of a discovery is the date of its first publication when that is known.")

Now while in many cases this principle may be not only a convenient one, but the only one capable of application, yet in certain cases it is a principle not to be followed at all.

These cases are those in which we have evidence regarding both the date of a discovery and the date of its being made known.

For instance, the date of the *discovery* of the circulation of the blood by William Harvey is certainly earlier than 1628, the date of its publication.

We have the evidence of Harvey's own lecture notes, dated 1616, that he knew of the circulation and was teaching it at least twelve years before he published it. Only when we cannot find the true date of a discovery should we fall back on the date of its publication.

Now the discovery of the existence of the invisible capillaries was a very great thing, and it seems a pity that we should get into the habit of assigning it to one year later than it actually was made.

Sir Michael Foster's account in his "Lectures on the History of Physiology" is clearness itself. Speaking of Malpighi, we read :—"Here" (Bologna) "he resumed office as a Professor of Medicine, and in spite of domestic troubles and anxieties, pursued his researches to such good effect that he was able in the next year, 1660, to announce privately to Borelli his discovery of the structure of the lung, an account of which was published in the year following."

The published account alluded to is his "De pulmonibus observationes anatomicæ," which, after the manner of the time, is in the form of two letters to his friend G. A. Borelli, at Pisa.

In the second epistle he describes the circulation in the herniated lung of the living frog. He heads the description with these words, "Magnum certum opus oculis video"—"I see with my eyes a great, certain thing," not, as always translated, "a certain great thing," which is much feebler and not a true translation.

1660, then, and *not* 1661, was the date of the discovery of the blood-capillaries, within only three years of the death of Harvey in 1657. Harvey made their existence a logical necessity; Malpighi made it a histological certainty. As we still speak of the "rete mirabile Malpighii"—for no lapse of time can ever make that rete less wonderful—we might as well take the trouble to assign the discovery of it to its correct date, 1660. D. FRASER HARRIS.

The University, Birmingham, June 19.

The Osmotic Pressure of Colloidal Salts.

CONGO red and similar salts in aqueous solution cannot diffuse through an ordinary dialysing membrane. When the osmotic pressure is directly measured by an osmometer it is found to be about that which the kinetic theory would ascribe to the salt present if the molecules were completely un-ionised.

The solutions, on the other hand, are good conductors of electricity, and the specific molecular conductivity of the solutions employed is that of a solute 60 to 70 per cent. of the molecules of which are dissociated. There is thus, as Bayliss has pointed out,[1] an apparent conflict between the results obtained by different experimental methods.

Congo red is the sodium salt of a complex organic acid. Its ions, therefore, are of very unequal size, and as the membranes employed in the osmometer are permeable to

[1] Paper read at the meeting of the Biochemical Society, May 4.

ordinary salts, they may be assumed to be permeable to the sodium ion, while holding back the much larger organic ion. On the other hand, it is found in actual experiments that the sodium ion does not escape from the osmometer.

Many collateral results show that the membrane does not *mechanically* directly constrain the sodium ion from diffusing. Thus a membrane, even when deeply impregnated with congo red, will permit the passage of sodium sulphate. Biltz and von Vegesack,[1] in order to explain the results, assume that the sodium ion of congo red, because it can diffuse through the membrane (and in spite of the fact that it actually does not do so), therefore contributes nothing to the osmotic pressure. I find myself quite unable to accept this view.

Osmotic pressure is a measure of the total constraint imposed on the system solution and pure solvent by a membrane. To the organic ions, owing probably to their size, the membrane offers what may be called a mechanical constraint. The sodium ions, on which the membrane exerts no such direct mechanical constraint, do not escape, because they are held back by the electrostatic attraction of the negatively charged organic ions. When equilibrium is reached, the electrostatic pull inwards must balance the osmotic pressure, which tends to drive the sodium ions outwards. The total pressure borne by the membrane, therefore, is the sum of the osmotic pressure of undissociated molecules, of the organic ions, and of the sodium ions held back by their attraction to the organic ions.

On this view, the osmotic forces would produce at the membrane an average orientation of plus and minus ions, which should appear as a contact potential difference between the solution and the solvent. The magnitude of the electromotive force can be calculated in several ways, of which the simplest is as follows.

If E be the potential difference of the quasi-condenser formed by the ions, P the osmotic pressure, and c the concentration of the dissociated salt in gr. equivalents per c.cm., then the osmotic force acting outwards on one gr. equivalent of ions is $\frac{1}{c}\frac{dP}{dx}$, and the electrostatic pull inwards is $\frac{dE}{dx}q$, where q is the charge on one gr. equivalent. Since these balance one another, we have

$$\frac{dE}{dx} = \frac{1}{cq}\frac{dP}{dx},$$

putting $c = P/RT$ we have

$$\frac{dE}{dx} = \frac{RT}{qP}\frac{dP}{dx},$$

which on integration gives

$$E = \frac{RT}{q}\log\frac{P_2}{P_1}.$$

This expression differs from Nernst's well-known equation only in the absence of the terms representing the rate of diffusion of the ions.

From what is known of colloidal solutions, it is possible that, except at a high dilution, some of the positive electricity might be carried by complexes too large to penetrate the membrane. These would contribute either nothing at all, or something less than the amount given by an equal number of sodium ions, to the potential difference. Their presence, therefore, would cause the calculated values to exceed the observed values. W. B. HARDY.

The Fox and the Fleas.

IN reference to the letters of Prof. Hughes in NATURE of March 23 and April 13, and his query as to whether the device adopted by foxes for divesting themselves of fleas is "instinctive," may I be permitted to give some particulars of a similar practice which has been observed in this country?

Foxes were introduced into Victoria from England in the early days of the colony, and are now plentiful even in the neighbourhood of Melbourne. Mr. P. R. H. St. John, of the Botanic Gardens, tells me that, whilst botanising or shooting in the neighbourhood of Point Cook during the 'eighties, he has on at least a score of occasions seen foxes enter the water with apparently the same object as the fox observed by Mr. Day.

Point Cook is situated on the west shore of Port Phillip Bay, and is about twenty miles from Melbourne. The surrounding country, being marshy, covered with saltbush, and of little agricultural value, was practically uninhabited, and a favourite hunting-ground for naturalists.

The procedure adopted by the fox was to retreat slowly into the shallow water (the beach being very level at that spot) until only the head was visible, and then it would disappear completely and rise to the surface about a yard away, and, leaving the water with a bound, the fox would rapidly reach the shore, shake itself like a dog, and make off into the bush. The time occupied by the whole operation, which was only attempted when the water was quite calm, would be about three or four minutes.

Mr. St. John, though never closer than about fifty yards, did not think that the foxes he observed held any wool or fur in their mouth (there were no sheep grazing in the locality), but he and his father, and various friends who accompanied him, had come to the conclusion that the object must have been to rid themselves of the fleas which were always to be found on those specimens which they shot.

It will be noticed that this manœuvre differs materially from that of the English foxes in that no wool or fur was used. This would suggest less call on the reasoning power of the fox, but on the other hand the deficiency was made up for by a final complete submersion and (there being no current) a side movement and a rapid escape from the water to dodge the dislodged and probably floating fleas.

The proverbial cunning of the race is surely to be seen in this adaptation of method to the conditions to be found on an uninhabited coast. HEBER GREEN.

Agricultural Chemistry Laboratory, The University of Melbourne, May 23.

Chemistry at the Forthcoming Meeting of the British Association.

MAY I direct attention to the following features in the provisional programme of Section B (Chemistry) at the British Association meeting in Portsmouth, beginning on August 30 under the presidency of Sir William Ramsay?

(*a*) Joint discussion with the section of agriculture on the part played by enzymes in the economy of plants and animals.

(*b*) Discussion on colloids (opened by Prof. Freundlich, Leipzig, with a contribution on the theory of colloids).

(*c*) Discussion on indicators and colour.

Many foreign chemists intend to be present—amongst others, Profs. Ostwald and Freundlich (Germany), Wegscheider (Austria), Gautier, Haller (France), Clarke, Barus (America), Righi (Italy), Pettersen, Euler (Sweden), Birkeland (Norway), Zeeman, Cohen (Holland)—and it is to be hoped that there will be a numerous attendance of British chemists to do honour to these and other distinguished guests. JAMES WALKER.

(*President, Section B.*)

Edinburgh, June 24.

Breath Figures.

LORD RAYLEIGH'S communication on breath figures (NATURE, May 25, p. 416) puts me in mind of an experiment—if I may call it so—we made when we were children. After breathing on a window-pane we wrote our names on the glass with the point of a finger. Now after having waited until the moist deposit had disappeared, and again breathing on the glass, the written characters became quite legible.

This seems quite to agree with Lord Rayleigh's explanation, grease on the fingers causing the phenomenon.

Delft, June 6. J. W. GILTAY.

[1] *Zeits. f. physik. Ch.*, 73. 481, 1910.